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Psychological Bulletin

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THE

PSYCHOLOGICAL BULLETIN

PROCEEDINGS OF THE NINETEENTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY, BALTIMORE, APRIL 22 AND 23, 1924.

REPORT OF THE SECRETARY-TREASURER, OWEN R. HUGHES, WEST TENNESSEE STATE NORMAL, MEMPHIS

Meeting of the Council of the Southern Society for Philosophy and Psychology, April 22, 11:30 A.M. Present: Buford Johnson, J. F. Dashiell, A. S. Edwards, Joseph Peterson, Josiah Morse, Knight Dunlap (acting secretary in the absence of Mr. Hughes).

(1) The following nominations were made, to be reported to the business meeting of the Society: President, J. F. Dashiell; Vice-President, A. S. Edwards; Members of the Council, Buford Johnson, and Josiah Morse.

(2) Thirty nominations for membership were approved and ordered reported to the business meeting of the Society.

(3) It was decided that the secretary should be instructed to have mimeographed the Constitution and list of members and officers, a copy to be sent to each member with the bulletin reprint.

(4) The secretary was instructed to notify the nominees approved by the Society immediately, to inform them that acceptance and dues for the ensuing year must be received before June 1; the election of such nominees as fail to comply before that date to be null and void.

(5) It was decided to recommend to the Society that a message of good wishes be sent by the Society to G. Stanley Hall.

(6) It was decided that the secretary should be instructed to have printed forms for the notification of members of arrears in

dues; also that the secretary should, as soon as possible, send notices to members two years in arrears, and should drop from the membership list those who fail to remit within thirty days after sending of such notices.

(7) A committee on program, consisting of the president and secretary, was appointed, to whom was delegated the arrangement of program for the next meeting.

Business Meeting, 9 A.M., April 23, President Johnson in the Chair.

(1) On nomination by the Council the following officers were elected for the next year: President, J. F. Dashiell; Vice-President, A. S. Edwards; Members of the Council, Buford Johnson and Josiah Morse.

(2) Thirty nominations for membership were made by the Council and were elected to membership.

(3) On recommendation by the Council a committee consisting of Professors Morse and Edwards was appointed by the Chair to draft and send a message of good wishes to Dr. G. Stanley Hall.

General Session, 2:30 P.M., April 23.

At the conclusion of the afternoon session, the following resolution was moved by Professor Crooks and unanimously adopted by the Society:

"Resolved that we, the visiting members of the Southern Society for Philosophy and Psychology, express to the Departments of Philosophy and of Psychology of Johns Hopkins University and of Goucher College, and to officers of these institutions, our great appreciation of the most generous hospitality which has been shown to us. The warm spirit in which we have been received has added much to the very pleasant meeting which we are this year having. The high spirit of serious intellectual effort and academic freedom which is traditional here has furnished the most fortunate possible background for our meeting."

ABSTRACTS OF PAPERS

A Comparison of Two Methods of Learning Gregg Shorthand. A. S. EDWARDS, University of Georgia.

Twenty-eight students, juniors, seniors, and a few sophomores were divided into two groups on the basis of the Army Alpha Intelligence Test. One group studied Gregg shorthand by means of the graded lessons in the manual; the other group studied by using from the start advanced material found in the Gregg Speed Studies.

Examinations were given at the ends of 18, 34, and 49 hours study, and included reading and writing Gregg, vocabulary, dictation and transcription. Only nine pairs of students finished the 49 hours study.

The small number of cases and the amount of variability do not permit of satisfactory judgments as to the values of the methods compared and the results are given only as suggestive at the present stage of the experiment which is to be carried further next year. So far as the work has gone, it appears that in general better results came from the use of the graded exercises. But those using the advanced material did better in writing shorthand.

"Spurious Correlations" in Psychology. J. Q. HOLSOPPLE, Johns Hopkins University.

A question is raised concerning the validity of using Pearson's correlation coefficient in measuring the relationships between series of psychological data on the grounds of the probable inclusion in r of one or more "spurious correlations."

The "spurious correlation," which arises whenever the measurements made in one series of data are included in any way in the measurements of a correlated series, has been known by mathematicians for many years to produce an error in the correlation coefficient which should be corrected by calculating the "spurious correlation" independently.

The following questions are asked: (1) Do psychologists use correlation coefficients which undoubtedly do involve a "spurious correlation"? (2) Is it possible that many of the correlations which we find are of this "spurious" variety even though they appear at first glance not to be? (3) Is it possible to avoid with certainty obtaining a "spurious" correlation? (4) Is it desirable that we should avoid it?

The first two questions are answered in the affirmative and the third negatively. The fourth question is left open with the suggestion that if "spurious correlations" are actually desirable as well as inevitable the correlation methods which are appropriate to this type of correlation should be used.

Experimental Isolation of Higher Level Habits. J. F. DASHIELL, University of North Carolina.

For exhibiting directly some of the higher-level part-habits

involved in a total habit, substitution material was found especially suitable, on account of the identifiability and easy control of the part-habits. College students were assigned a series of trials on a digit-letter substitution test. The group working with the same code (identical part-reactions of all levels) from trial to trial showed most rapid learning; that working with a code changing completely from trial to trial showed a fair rate of learning; while the group using a code partly new and partly old showed great interference effects in the form of a retarded rate of learning. The graphed results showed clearly that the acquisition of an entire complex of habits—a story of integration into a permanent unified hierarchy—proceeds clearly at an acceleration superior to that attainable in learning only the higher-level and not the lower-level detailed responses; that interference effects are producible by a succession of situations in which the low-level responses aroused are partly new and partly old; and that when a new situation is presented those practiced throughout on one identical habit-complex are at a disadvantage.

Variability in Response in Two Types of "Synonym-Antonym" Test. D. W. SEAGO, Johns Hopkins University.

A study was made of the variation in responses to the individual pairs of words in two forms of "synonym-antonym" test, with the purpose of determining some of the factors conditioning the responses. In the first form of the test only synonyms and antonyms were included. The second form, which was given two months later, included pairs of words that were neither similar nor opposite in meaning, together with some of the synonyms and antonyms used in the first form. It was found that the percentage of error when the "neither" group of words was introduced was considerably greater for some pairs of words and less for others, whereas, according to chance alone, it should be slightly greater for all of the pairs. It is assumed that for the total number of pairs of words not known, there will be a chance distribution of errors and correct responses in each form of the test, and a definite proportion of errors common to both forms. The percentage of errors common to both forms was found to be considerably less than was expected according to chance. A study of the types of errors reveals certain definite tendencies, such as the tendency to judge the easier synonyms (as measured by the percentage of accuracy) as opposite in meaning rather than as

neither similar nor opposite, whereas the more difficult synonyms are more frequently judged to be neither similar nor opposite in meaning.

A Comparison of Lignac's Doctrine of the Self with that of Maine de Biran. G. C. BUSSEY, Goucher College.

The paper attempts to show that the thought of Maine de Biran which is now winning recognition as one of the determining influences in the movement of recent French philosophy away from positivism, is identical in certain important respects with the thought of the Abbe de Lignac. Lignac was an obscure but independent follower of Malebranche and Descartes, who in the eighteenth century upheld the rationalist tradition against the assaults of sensationalism and determinism.

The agreement in the views of these thinkers lies (1) in their method, and (2) in their conclusions. Both maintain that their method is essentially empirical and that any thoroughgoing empiricism must include the consciousness of the self. This consciousness of the self is not only one element in experience, but the basis of all sane philosophy. For both the self as immediately experienced is active, free, a unity back of the multiplicity of its changing states. Without the consciousness of the self there could be no sensation, no recollection, and no judgment. While both teach that the self is preëminently free, de Biran goes further than Lignac and maintains that free activity, *i.e.*, the putting forth of effort, is identical with the self. Lignac believes that we could will even though we could not move, while de Biran teaches that if we could not move there could be no consciousness of the self or of freedom. For both freedom is the presupposition of the intellectual and moral life. As immediately experienced, freedom is identical with the sense of effort, of activity, but does not afford knowledge of the bodily processes through which volition is carried out. Both Lignac and Biran believe that the consciousness of the self brings with it an immediate consciousness of the body, which is the condition both of the localization of sensations or feelings within specific parts of the body, and of the certainty of the existence of external objects. For Lignac the consciousness of body is the consciousness of a certain portion of matter which coexists with the self; for de Biran it is primarily the consciousness of a resistance or inertia to be overcome.

One of the most important points of agreement between their

views is that both Lignac and de Biran derive all the fundamental categories of thought from the immediate consciousness of the self. Causation is essentially the consciousness of activity as contrasted with passivity, and must be distinguished from mere temporal sequence. According to both, there is no real cause but will. The categories of unity, identity, substance, mode, necessity, contingency are both based upon the consciousness of self and of body.

For Lignac, the consciousness of the self brings with it a direct consciousness of God. For de Biran, on the contrary, God can be inferred to exist as cause, but can not be directly perceived.

In pointing out this agreement, I have made no attempt to establish Lignac's influence upon de Biran. De Biran was familiar with Lignac's writings, but there is no evidence that he derived his ideas from the earlier thinker. The point of interest is that those French thinkers at the present time who base their certainty of the self as free upon direct awareness, are carrying on a thought which is prefigured in the thought of this little known philosopher of the eighteenth century.

A Pragmatic Absolute. E. B. CROOKS, University of Delaware.

Apparently it has been assumed by many thinkers that the concept of the Absolute may be discarded because proofs of the existence of the Absolute, satisfactory to them, have not been adduced. It may well be that this is not sufficient grounds for the rejection of the Absolute.

If the Absolute proves the best concept for a final organization of our thought then we are under the same obligation to use such concept as we are to seek such final organization of thought. This would, of course, get us no further than an epistomological Absolute.

Again, if in seeking an explanation of reality we find ourselves unable to come to rest short of total unity then our thought necessarily takes the form of some type of absolutism. Such an Absolute would have as much validity as our thought urge can give. Such thinkers would be obliged to accept the metaphysical reality of an Absolute.

If our ethical thinking is of the type which demands a fixed standard of reference, then we are not able to stop short of an universal standard, namely, an Absolute. With Kant we postulate

such a principle of value, even if we are unable to prove its objective existence.

And on pragmatic grounds why should we not do all of these things? If the acceptance of the Absolute gives us confidence in truth, furnishes us a metaphysical principle of explanation and gives us a bulwark for ethical values, in short, works well in our philosophy, is such an Absolute not worthy of acceptance? On what pragmatic grounds can we reject it? Such a philosophy is fit to be believed, by one who has seriously tested it and finds no power in himself to accept another.

Some Recent Criticisms of the Coherence Theory of Truth. R. P. HAWES, Goucher College.

Most of the criticisms which have been directed against the coherence theory of truth by the English New Realists, by A. K. Rogers and by L. A. Reid miscarry, in some cases because of faulty inference, but chiefly because based upon misconceptions of the coherence theory. There are three criticisms, however, which are not so easily disposed of, namely, that the coherence theory (1) does not distinguish between truth and reality, (2) cannot explain error, and (3) leads to relativism and agnosticism. But to these three objections the adherent of the coherence theory may reply: that the coherence theory (1) does not necessarily involve an identification of truth and reality, (2) has less difficulty than other theories in explaining error if it supposes an active, interpretative mind in direct contact with a reality distinguishable from it, and (3) does not lead to a hopeless, self-contradictory relativism and agnosticism which denies the possibility of obtaining even a degree of truth, but implies only the impossibility of the finite mind's getting the infallible, complete, and self-complete.

Some Experiments in Mental Set. J. F. DASHIELL, University of North Carolina.

Individuals differ from each other in the quickness with which they get into the mental set for a piece of work. They differ also in their ability *in* the work. Do these two lines of difference parallel each other; is he who most quickly gets *into* a task likely to be the fastest *in* that task? The question was approached with the use of printed arithmetical examples and group intelligence tests, each arranged in two forms, the continuous and the mixed.

Definite negative correlations were found between ability in a task and quickness in shifting from one task to another. More elaborate studies of the problem are in progress with apparatus setting distinct kinds of tasks for right hand, left hand, and feet.

Some Factors Involved in Color Preference. R. M. DARCUS, Johns Hopkins University.

The problem under investigation was to be the determination of the effect of brightness and saturation upon the choice of colors with consideration of sex differences and age differences. Another point considered was the influence of association upon choice. Data were obtained from testing 400 college men, 400 college women, 300 children (ages 8-10), 25 aged women (over 60), 20 aged men (over 60), and about 75 cases in a psychopathic hospital. The method employed was by paired comparison and the material consisted of six standard colors of unequal brightness and unequal saturation and the same six colors of equal brightness and equal saturation. Associations were secured for both the equated and the unequated colors. Definite age differences are found but a completion of the study is necessary for determining the actual differences throughout the range of ages considered, as the results formulated are based upon a partial compilation of data. Sex differences are indicated in the order of preference in both series of colors. Also the types of associated words are influenced by the sex factor for the various colors.

A Functional View of Consonance. J. PETERSON, Peabody College.

Consonance cannot to-day be regarded as a simple characteristic of simultaneous tones and the experience of consonance by anyone is not an "elemental sense". Several factors contribute to the degree of consonance or dissonance of most intervals, some being more prominent in one case and others in another. These factors are roughness due to beats, habituation, nearness of primaries to the characteristic difference tone of ratio 1, and possibly others.

The first serious attempt at scientific explanation of consonance and dissonance was made by Helmholtz, who made the roughness of beats or the absence of this quality the main determinant of degree of consonance. According to this theory dissonance is due to the interruption of the smoothness of the tonal complex caused by beats of primaries, upper partials, or difference tones,

or of any combination of these. The beating of difference tones he held to be the most general cause. This last fact has strangely been neglected by most recent writers. This theory has been amplified by Krueger in two important respects. Krueger puts great emphasis on difference tones, especially of ratio 1, and on the complexity of the whole tonal mass formed by these tones and by the primaries. "Explanations" by Stumpf and by Lipps as well as the more recent theory of Watt are little more than mere description in other terms of what we experience, and leave no adequate basis for empirical tests of hypotheses.

The position there taken agrees with that of Helmholtz and of Krueger, but stresses the point that consonance is not based on a fusion of once discrete elements which have been unified by habit as the association theory has implied, *i.e.*, by the greater frequency of occurrence of our present consonant intervals than of the dissonant ones; on the contrary, it stresses the general psychological principle that we preceive or react synthetically to stimuli that have always or even habitually occurred together, such as the partials of a clang or of the "voice" of any well known person. We do not react to the several elements as distinct factors and then combine them by later synthesis based on habit, as Ogden's theory seems to imply, though he has slightly improved it in his recent book. His theory is not yet clear of implications or the transmission of somatic changes. Experiments on the conditioned reflex show that simultaneously variable stimuli are reacted to as unitary. Now tones making simple ratios of 1:2, 2:3, 3:4, 4:5, 3:5, etc., form with their characteristic difference tone, 1, a complex very similar to that of the clang partials, *e.g.*, (1), 2, 3 for the fifth, (1), (2), 3, 4, for the fourth, etc., and so these are perceived as unities in a manner quite similar to that of the clang. I have enclosed in parentheses the difference tones. The difference between such complexes and those of partials of a clang is simply one of relative intensities. Two or more difference tones, for such simple ratios, will usually coincide for the characteristic tone and thus give it increased importance. Considerably mistuned intervals will be rough and unpleasant. More complex ratios, as 8:11, will not form complexes similar to clangs because the primaries are so far removed from the characteristic tone and not all intervening harmonics of 1 will be present, if indeed 1 itself is present. Frequent use may easily bring 8:11, however, within the range of consonance because

there are no offending beats. Experiments show a rapid gain in favor of such intervals with habituation, also it is found that thirds, *e.g.*, above the range of habituation are judged less consonant than thirds within this range. Instead of Ogden's inheritance of habits, the present view suggests the importance of habits being objectified in the form of musical instruments, written scales, etc., which condition fixes on the individual the experience of the race, but in the form of stimuli not of neural organization.

Various attitudes of tonality and of other factors affect subjects reacting to Seashore's consonance test, and different subjects tend to emphasize different elements contributing to consonance. While this does not destroy its considerable degree of practical usefulness, its validity and reliability need careful study.

Two Forms of Analysis. H. E. CUNNINGHAM, West Virginia University.

The two forms of analysis, experimental and intellectual, are developed. The former prevails in the realm of science proper, the latter in the realm of knowledge. The subject matter of the former is the world of nature, of the latter, the world of knowledge. The former begins with objective-nature facts and seeks to introduce the relation of cause, the latter begins with knowledge facts and reduces these to the fundamental categories of *terms* and *relations*. The two types of analysis thus furnish two types of explanation or two *levels* of explanation. Difficulty and confusion result from the failure of one engaged in one type of analysis to enter into the spirit of one engaged in the other type. Many of the disputes between pragmatists and realists might be replaced with more fruitful philosophic contributions by the recognition by each that the other is engaged in a different field of endeavor and in a different level of explanation.

Report on a Series of Ten Tests Used with Immigrant Children at Ellis Island, with Special Reference to a Color Test. B. M. BOODY, Johns Hopkins University.

The ten tests, which were given to a group of children, ranging in age from three to nine years, representing some twelve different nationalities, were as follows:

(1-5 Nonlanguage tests from the Binet Scale): 1. Drawing of Square (Binet 4); 2. Drawing of Diamond (Binet 7); 3. Divided

Rectangle (Binet 5); 4. Forms (Binet 4); 5. Bow Knot (Binet 7); (6-10) include four (6, 7, 8, 9) which Mrs. Woolley is using with little children in Detroit; 6. Drawing of circle, cross, star; 7. Touching with thumb, each of four fingers in succession; 8. Building of Pyramids; 9. Button holes; 10. Color Test.

Number 10, the Color Test, was devised by the writer specially for this work, as no existing test exactly fitted the requirements.

The results of the Binet tests used, showed in all except No. 3, which offered such difficulties in explanation without language, that the results must be discontinued, the comparative exactness of the placing of these tests in the age scale for all the nationalities in the group.

Tests 6, 7, 8, 9, with the exception of two individual cases, seemed to show age differences. Test 10, the Color Test, was first used with 60 older children (age range, 8-10), then in an abbreviated form with 22 young children.

Sixty forms (circle, square, triangle, octagon, diamond, each in three sizes and four colors) were placed in a standard order, before the child, to whom the experimenter showed in turn 10 samples to be matched from among the sixty, as to color, form, and size. Time and response were recorded. For the older children, no apparent racial or age or sex differences appeared in the small number tested.

Errors were made in the following decreasing order: Size: Form: Color. There was suggestion of possible color blindness in but one case.

For the little children (where but four samples were used to be matched from a field of twelve), errors in color and size were equal, exceeding those in form.

As with the older children, the differences here, too, seemed to be individual differences.

Determinism and Consciousness. W. C. RUEDIGER, George Washington University.

The purpose of this paper is to point out the place of a functional consciousness in a monistic conception of reality. The heart of the situation is revealed in associative memory. It is in associative memory that past experiences are brought to bear on present situations. We usually say that past experiences when used deliberately in guiding conduct functions *in* consciousness. It is right there that we go wrong for this is the language of dualism. Uncritically we

assume that consciousness is something in which past experiences, stored in the nervous system, play their rôle. Why not say that this functioning of past experiences *is* consciousness? Whenever an organism has evolved to the stage where retained experiences are used in guiding present reactions we have consciousness. This consciousness, viewed monistically, is not something that is also there, it is part and parcel of the process. It is one aspect of the way in which nature operates on this particular stage of organization. You can no more remove it without destroying the functioning than you can remove the flame without destroying the combustion or the hardness without destroying the diamond.

Fundamentals in Psychotherapy. T. A. WILLIAMS, Washington, D. C.

The paper discussed fundamentals in psychotherapy, criticising adversely the use of indiscriminate suggestion as introduced by Bernheim of Nancy over thirty years ago, and more recently exploited by the combination of suggestion with the overworked notion of an unconscious mind.

"Suggestion is only a disingenuous inculcation of what we want people to believe no matter how." A self-governing people must learn to withstand suggestion. Those who find no truth in themselves pessimistically preach that evasions and conformities are the salt of life." A fundamental psychotherapy must eschew such props and go to the root. To do this self-examination by the patient must be insisted upon. Everyone should know himself without evasions. But "so-called repressions are not responsible in themselves for psychoneuroses", nor need inward struggles concern sex alone.

"The mental processes responsible for psychic illness are not unconscious"—absurdities wherein the doctrines have been so popular.

The way "to remove mental symptoms is to give to the patient an understanding of the mental process which is occurring". That is a true psychodiagnosis very different to giving a catch word.

Sometimes the illness is of bodily origin, in which case it should not be treated by mental measure but the physical cause dealt with by hygienic, medical or physical measures. A combination of mental and physical factors is frequent; hence "psychic difficulties should be managed only by persons well versed in body physiology as well as psychology, that is specially trained physicians".

A Radical Modification of the Maze Problem. J. F. DASHIELL, University of North Carolina.

The maze is a much-used form of experiment in learning, but is not completely adequate for bringing out details of the learning process because results on individual subjects are found to be very irregular and not readily comparable. This is attributed to the fact that the maze does not present the possible errors in a serial or one-way pattern—as is true of all other learning experiments—but makes the subject more liable to recommit old errors after committing a new one. To check this insight a white rat maze was used equipped with sliding doors that were closed behind the animal as it progressed. Comparison of results with those on the old type of maze showed that the use of doors both reduced the time taken for learning and reduced the irregularity in the learning process.

Contemporary Philosophy and Psychology: Their Common Problem. A. O. LOVEJOY, Johns Hopkins University.

Though, except in the Southern Society, philosophers and psychologists have in recent years been more and more segregated from one another, certain tendencies in recent American and English philosophy have increasingly brought into the center of interest in philosophical discussion a question to which psychologists can scarcely be indifferent—since it may be phrased as the question: "What are psychologists really talking about? How, in other words, if at all, do the events or entities which they investigate differ from those investigated by the sciences called physical? The prominence of this problem in recent reflection is a natural consequence of the revival of realism. A realistic philosophy is at once compelled to face the issue concerning dualism: is it to construe its fundamental thesis as involving the admission that there are two worlds made up of utterly different kinds of stuff; and if so, how can the existence and nature of the one be made known to us through the other? And the characteristic impulse of scientific theory towards unification and simplification has caused much contemporary realism to seek, in many divergent directions, a way of escape from dualism. Attempts to restate, attenuate, or eliminate, the psychophysical distinction have consequently been especially characteristic of recent philosophy. The prevalent habit of simply reformulating the distinction, of offering new definitions of the "nature of

consciousness", is a pernicious one, since clarity and intelligibility in discussion require that words shall have reasonably fixed meanings and that debate shall be concerned with factual propositions expressed by such definite terms. To say that "consciousness exists", but to mean by "consciousness" something that nobody ever meant by it before, is to darken counsel; and it often leads to the crude fallacy of assuming that any "consciousness" not corresponding to one's new definition has thereby been proved to be nonexistent. But attempts actually to reduce or eliminate the psychophysical distinction, by more than merely verbal arguments, are at once to be welcomed as a normal expression of the philosophic craving to unify, and to be sceptically subjected to the severest logical tests, applied through methodical and coöperative inquiry on the part of both philosophers and psychologists.

In this task of examining at once sympathetically and sceptically the proposal to qualify, narrow, or discard the division of real things into two fundamentally distinct classes of mental and physical, it is important to begin at the right point. That point lies in the elementary and now familiar, yet long neglected or blurred, distinction between two aspects—or possible aspects—of the life of the human organism. The rejection of the psychophysical distinction, namely, may be attempted either in the description of what the organism *does* in, *e.g.*, perceiving—the activity, state or process in which that function may be supposed to consist; or in the description of what the organism *has*, as its experienced content. The worst and most fundamental error of reasoning which can be made in the legitimate endeavor to reduce the "psychical" to the physical, is to suppose that in giving a purely physical description of the organism's activity *in experiencing*, one has *thereby* given a physical description of its experienced content. This is the primary error of behaviorism; it is so obsessed with the question what the organism does that it forgets the problem of content altogether. The two problems cannot, on a physically monistic basis, be identified, or solved by the same considerations, for the obvious reason that, physically, the organism's activity is intracorporeal, while its content of experience, if physical, is chiefly extracorporeal, is "out-there" in space, and often also remote in time. Let the activity be as strictly behavioristic as you please, the content—which is the real stuff of actual experience—remains unaccounted for, and, indeed, unmentioned.

As the second step in the orderly prosecution of this inquiry it can be shown to be impossible to reduce *both* the process of experience and the content to physical events—*i.e.*, to motions of bodies. This is what is attempted by the neorealists of behavioristic tendency (*e.g.*, Perry); on the one hand they insist not only upon the reality of content, but also upon its literal identity with physical objects external to the body of the percipient organism; on the other hand they identify its being perceived with the occurrence of certain movements of or within the organism. But a description of the motions, simultaneous or successive, of two groups of protons and electrons in different parts of space, is *not* a description of the event which consists in one of those groups being experienced by, or in connection with the motions of the other. It may be a statement of a necessary condition or cause of that event; but it does not so much as mention that the specific phenomenon of being-perceived has happened. A neorealist, surely, is bound to hold that one physical thing can be compresent in space, and move with any assignable direction and velocity, along with another physical thing, without being *eo ipso* perceptual content for the latter. Thus the conjunction of neorealism with behaviorism is an especially odd union of incompatibles.

Any realistic formulation of the psycho-physical distinction should (a) start from the contrast between the things that at any given moment are, and the things that at that moment are not, content in the experience of a given organism; (b) should state precisely wherein the difference between the two consists; (c) should recognize that the acquisition, by objects outside the percipient organism's body, of the status of being perceptual content is at any rate *not* the same fact as any "behavior," *i.e.*, movement of that body. The second of these requirements is disregarded by several recent reformulations of the distinction, *e.g.*, Hollingworth's conception of a psycho-physical continuum, in which the two "orders," mental and physical, differ merely by their relative frequency of occurrence. This is a division of content into two classes, not a statement of the difference between content and noncontent; it does not put the distinction at the point where it is needed.

These are merely the necessary first steps in the inquiry; but they establish the conclusion that at least one of the two components of experience—*either* experiencing or content—must be described in terms other than descriptions of motions of matters. The present analysis, however, leaves unconsidered the ulterior and more com-

plex question whether one or the other of the components may not be exhaustively describable in such terms.

Symposium on Philosophy, Psychology and Religion.

PROFESSOR JAMES H. LEUBA, Bryn Mawr College.

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MEMORY[1]

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I. MEMORIZING

Effects of Oral Reading and Recitation upon Memorizing. Woody (**66**) has shown an advantage for oral over silent reading in the memorization of poetry. Efficiency was measured in terms of the time required to memorize. The conclusions are supported by the comparisons of five pairs of poems. In four comparisons the advantage is in the direction stated. When efficiency is measured by repetitions required for memorizing no such advantage appears for oral reading. This is due to the fact that a certain amount of wandering of attention and regressive reading prolonged the repetition during silent reading. Using both nonsense and sense material Skaggs (**53**) found it better to intersperse readings and recitations than to group the readings together and the recitations together. Although he accepts the earlier findings of Witasek, Gates, and others as to the advantageous effect of introducing a certain amount of recitation into the learning, he has a word of criticism which seems to the reviewer well taken. He points out that the attempts to recite have in the earlier experiments been complicated by the fact that promptings have been given where the subject was unable to recite. "So," he says (p. 425), "they have not compared mere L (reading) with mere R (recitation), but with a mixed method which complicates the total process a great deal." Fortunately, however, it is not necessary to reject the general implication of their results on any such account.

Part and Whole Methods. Reed (**42**) claims that the conclusion so frequently drawn that the whole method of study is better than the part is not justified. While recognizing that the experiments of Lakenan and of Pyle and Snyder favor the whole method, he says that certain results obtained by Pechstein point in the other direction. Meumann, according to Reed, drew conclusions not supported by his

[1] This review of memory studies since 1920 will consider only those of an experimental or empirical type.

own results. In order to prove this point he gives certain data from Ebert and Meumann which show an advantage for each of a number of varieties of the part method as compared with the pure whole method. A repetition of this experiment by Reed also favored the part methods. Three individual experiments out of four conducted by Reed show an advantage for the part method. Poetry and prose were used here. In another experiment with poetry, in which Reed employed 113 subjects, the part-progressive method proved best, the part next best, and the whole poorest.

The issue is plainly in a very unsettled condition. Perhaps the question as stated is too large for a single generalization. In that case it is time to proceed to an examination of why the whole method is advantageous when it is advantageous and why the part method is advantageous when it is advantageous. Woodworth in his *Psychology* has pointed out that interest, confidence, visible accomplishment, and recency are on the side of the part method, while meaning, outlining, and permanency are on the side of the whole method. A further experimental problem is that of discovering when interest and confidence may profitably be saved at the expense of that continuity of meaning which the whole method insures.

Distribution of Study. Pechstein has recognized in the realm of so-called motor learning the intimate relationship between the part-whole problem and that of the optimal distribution of practice. A similarly intimate relationship probably exists in the realm of memorizing. One experiment conducted by Schmitz (49) might very properly be considered in connection with either or both of these problems. The experiment referred to was of the memory span type. Lists of 6 or 7 nonsense syllables were presented with a pause of one second between the third and fourth or between the fourth and fifty syllables. This pause was beneficial. Austin (4) also found some advantage in scattering the readings of logical material taken from mathematics, history, and embryology. Robinson (45) obtained results which hint at disturbing complexities underlying this general problem. He found an advantage in dividing 12 presentations of a list of ten three-place numbers into 2 sets of 6 readings each engaged in 24 hours apart. But the advantage was not so apparent when 6 presentations of lists of the same length were divided into 2 sets of 3 readings each engaged in 24 hours apart. In fact, as measured by recall 5 minutes after the close of study, a concentration of all 6 readings into the same period appears better.

Order of Presentation. Winzen (63) sought to discover whether, when two things are to be associated, it is advantageous to present the better or the less known member of the pair first. He began with experiments in which nonsense syllables and words were associated. Learning was more efficient when the word of a pair was presented, and after it the syllable, than when the opposite order was employed. This superiority for the word-syllable order also stood up when subjected to a backward association test. In another series of observations pairs of syllables were employed, one member of each pair being made better known than its fellow by virtue of the fact that it was given a preliminary reading or to the fact that it was read twice instead of once. In 5 out of 5 experiments superiority is shown where there is a primacy of the better known syllable. A final investigation was made in which the first syllable of each pair was written large and in red. Learning was more efficient when the large, red syllable preceded its more ordinary associate. Roback and Groetzinger (43), working with names, got results directly opposed to those of Winzen. They found that a combination of names possesses greater memory value if the more familiar component appears after the less familiar. H. F. Adams (3), on the other hand, conducted an experiment which lends support to certain of the Winzen findings. He used full, half, and quarter page advertisements. Sometimes the smallest advertisement of a given firm was presented first and sometimes last. The anti-climactic order showed an advantage of 11+ per cent.

Moers (36) presented syllables or letters in three different ways: (1) from below upward, (2) from left to right, and (3) auditorily. The first method, as one might expect, was to begin with considerably poorer than the other two. The left-to-right and the auditory presentations were about equally efficient. The first method improved more from practice than the other two, another fact which might have been expected.

Size and Color Variation. Peterson, some years ago, studied the effects of size and color variation upon the difficulty of lists containing 9 syllables each. The results showed practically no difference between lists whose syllables varied in color and those whose syllables were constant in color. Size variation among the syllables of a list did seem to make that list easier. Achilles (1), using 16-syllable lists, studied the effects of size and color variation, but she was unable to obtain any significant differences. The paper of

Floors (16) contains important facts in regard to this topic, but, since he was mainly interested in the effect of size and color variation on retention and recall, we shall postpone our discussion of his work until later in this review.

Determination to Learn. It has, of course, long been known that the determination to learn has a pronounced influence upon the rate of learning. Achilles has refined our knowledge upon this point. She has demonstrated that the difference between determined and undetermined learning shows up more plainly when amount of learning is measured by recall than when it is measured by recognition. She has also demonstrated that the difference in amount learned as measured by recall and the amount learned as measured by recognition is greater where determination is lacking. Shellow (52) has a special monograph on incidental memory. Individual subjects vary less in direct memory than in incidental and the amount of variation in incidental memory depends upon the material. Incidental memory is poorest for disconnected material and material least closely related with the primary direction of attention.

Learning to Recall versus Learning to Recognize. Achilles obtained ratios of repetitions required for recall to repetitions required to recognize.

Material Learned	*Ratio*
Pictures	3.23
Forms	2.20
Words	1.80
Syllables	1.22

It is concluded that the richer the associations involved, the greater the difference between the repetitions required for recall and those required for recognition.

Rate of Presentation. In an experiment conducted under the direction of Rich (13), the relationship between rate of presentation and amount memorized was studied. The real interest of these investigators apparently was not so much in the memory process as in the possibility of treating the data of a memory experiment by means of the refined statistical procedures of psycho-physics. The reader may remember that a step in that direction had earlier been taken by H. D. Williams. In the Rich experiment the aim was to determine by the best possible mathematical procedure that rate of presentation which would yield 50 per cent correct recalls by the *Treffermethode* after 5 repetitions of 5 pairs of syllables. The rates

varied with one subject from .5 sec. per syllable to 1.5 sec. per syllable and with the other from .5 sec. to 1.10 sec. per syllable. In general there is increasing recall with decreasing rate of presentation. But this is not strange, since as soon as it was discovered that the second subject did less well at the 1.25 sec. and 1.5 sec. rates than at that of 1.0 sec., those slower rates were discontinued for that subject. The arguments for discarding the slowest rates are that they give results in disagreement with what has been found by others and that, at those slow rates, a new factor of wandering attention was introduced. Are we to believe that attention wandering enters the situation suddenly after a given rate of presentation has been passed? Inversions of the second order appear in the functions for both subjects.

Fineman and Ruderman (15), also working under Rich's direction, compared four constant rates of presentation, ranging from .5 sec. to 1.0 sec. per syllable, with a rate of presentation which was accelerated from 1.0 sec. per syllable during the first of four repetitions to .5 sec. during the last. Meumann had earlier discovered that a learner, if left to his own devices, would accelerate his rate of study, but, as the present investigators remark, this fact throws light upon the most comfortable study method and not upon that which is most efficient. Fineman and Ruderman found that the accelerated rate of presentation gave a degree of efficiency somewhere between that given by the best and that given by the poorest constant rate of presentation. The best constant rate, when considered in relation to total learning time, varied for the two subjects employed.

Length of Material. Robinson and Heron (47) have called attention to the fact that the relation between length of material and effort required to learn has by many writers been expressed as a relation between length and repetitions. Only two or three seem to have realized that *one repetition* is a unit whose magnitude varies directly with length of material. Results of Ebbinghaus, Meumann, Binet and Henri, Henmon, and Lyon are presented. When effort required to learn is measured in terms of constant units, *e.g.*, learning time, the relation between length and difficulty usually shows a positive acceleration. That is, as length increases difficulty increases at an increasing rate. This finding is particularly clear in Robinson and Heron's own work with nonsense syllables and also in another paper, by Robinson and Darrow (46), in which an experiment utilizing 3-place numbers is reported.

Fatigue. Morgan (38) had 5 high school students who knew no German partially learn the English equivalents of 850 German words. The words were grouped into lists containing 10 pairs each, and each list was shown 4 times before the next was presented. After the first presentation of a list, the subject tried to fill out the pairs, each of which during the second, third, and fourth presentations appeared with only its German word for two seconds before the English word was supplied. This is a form of the so-called *anticipation* or *prompting* method. The total study time for the 850 pairs amounted to approximately 4 hours. The average learning scores for 5 consecutive sections of the learning period were 3.62, 3.36, 2.99, 2.96, and 3.47. It would seem from this that memorizing capacity is affected to a remarkably slight degree by prolonged and continuous use of such capacity. That this conclusion is not justified, however, is apparent in the fact that these subjects had had no preliminary practice in memorizing material of this sort. Anyone who has ever performed a memory experiment knows that the effects of practice upon learning ability is very great, and in Morgan's experiment this practice effect must have tended to obscure the fatigue effect. The only justifiable conclusion left is that the fatigue effect was not great enough to obscure the practice effect. But such a conclusion means little unless some prediction can be made of the magnitude of the practice effect. In this experiment such a prediction cannot be made.

The Curve of Memorizing. Kjerstadt discovered that the curve of memorizing is independent of the length of material learned, providing the curve is constructed on a basis of relative units. What he actually did was to divide the total time required to learn a list into a certain number of parts and then to calculate the percentages of the total material learned which were learned in those selected parts of the total learning time. Curves so derived for lists of material of various lengths were practically coincident. Robinson and Heron, working with lists containing 6, 9, 12, 15, and 18 syllables, got an apparently complete confirmation of Kjerstadt's law. The methods of calculation employed were not, however, identical with those of Kjerstadt. This is pointed out by Robinson and Darrow who have discarded the methods used by Robinson and Heron and applied to the nonsense syllable data, as well as to new data from lists of 4 to 10 three-place numbers, the methods of calculation originally employed by Kjerstadt. The results again show coincident relative curves for the syllable lists ranging from 9 to 18 syllables in length and for

number lists containing from 6 to 10 numbers. The law does not hold, though, for the shortest lists of either syllables or numbers. Perhaps further experimentation would reveal an upper as well as a lower limit to this law.

It is claimed in the Robinson-Heron paper that Kjerstadt, while considering these relative curves for materials of different length, should not have neglected the far simpler and more intelligible comparison of the curves of memorizing constructed on the basis of constant units. In the Robinson-Heron and Robinson-Darrow papers such comparisons are made. Both the syllable lists and the number lists give fairly regular sets or curves. The general tendency in all cases is toward negative acceleration. The shorter the lists the more rapid their initial rise.

In the first of the papers cited above as having been written under Rich (13) there are results on the relationship between learning time and amount memorized. The reviewer is somewhat surprised, considering the number of observations made and the thorough fashion in which practice was eliminated, that one of the two curves of memorizing secured was not more regular.

The generalization made a few lines back that the shorter the list the more rapid its initial rate of learning may be given a certain *a priori* limitation. It is obvious that a given learning time would not continue to give larger and larger returns if the amount of material studied were indefinitely diminished. In fact, if the proper lengths were hit upon it would be possible to have an experiment in which the longer material had a more rapid early learning rate than the shorter. This possibility has been fulfilled by Michel (35), although he does not discuss his own results from this angle. This investigator presented from 10 to 20 common objects. Two exposure periods were employed, one of 10 sec. and one of 5 sec. Amount learned was scored in terms of both recognition and recall. The more material presented, irrespective of the exposure time, the more recalled and recognized.

Associative Inhibition and an Extension of the Ranschburg Phenomenon. Kline's paper (29) describes certain experiments on the law of associative inhibition. The aim was to discover the degree to which this law holds for meaningful material. The law was formulated thirty years ago by Müller and Schumann. It asserts that when any two elements become associated it becomes more difficult to form a new association between one of those associated

elements and a third. "If *a* is already connected with *b*, then it is difficult to connect it with *k*; *b* gets in the way." In Kline's experiments states and countries were associated with false capitals, counties with false county seats, books with false authors, and pairs of numbers with false sums. Two sorts of check tests were used. Either unknown material was associated or, by tests of current knowledge, the strength of relevant existing associations was determined. The results indicate that if *a* is slightly but not strongly associated with *b*, it is readily associated with *k*. If *a* is strongly associated with *b*, its association with *k* is inhibited to a considerable extent. But if *a* and *b* are very strongly associated, *a* may be very easily associated with *k*. Kline says that the difference between old and new postal clerks in regard to learning a new distribution for a mail route illustrates his finding. The old clerk forms the new associations more readily in spite of the fact that he has more already formed associations which well might cause associative inhibition. This exception to the law of associative inhibition which Kline has demonstrated for meaningful material should be followed up with meaningless material where the possibilities of intensive analysis are usually so favorable.

Experiments conducted by Schmitz (49) have extended the importance of the Ranschburg phenomenon. Ranschburg had found that the repeated member of a presented list tends to be omitted in recall. This is claimed presumably only for the memory span type of experiment. Thus, if such a list as a, b, c, d, b, e, f, g were presented, b, the repeated element, would tend to be omitted in recall. Schmitz found that the unfavorable status of the repeated element extends to the element following it, even though the Ranschburg phenomenon is absent; that is, even though the repeated element is not, itself, omitted in recall. It seems likely that both associative and reproductive inhibition are involved in the Ranschburg phenomenon and in the extension of it which has been demonstrated by Schmitz.

Group Differences and Correlations. Achilles found that women are better memorizers than men as measured by both recall and recognition of words, forms, and syllables. Girls also proved better than boys. No sex differences in variability appear in Achilles' results. In the same investigation there is shown to be an increasing memorizing capacity with increasing age. This is consistently true whatever the material memorized (words, forms, or syllables) and whatever the form of the test (recall or recognition).

Persons who recall one material well may not recall another well according to Achilles. While the correlations are slight, they do tend to be positive rather than negative. The correlations between memory as tested by recall on the one hand and by recognition on the other are also low, but usually positive. Woody obtained positive and fairly high (.5 and above) correlations between time or repetitions required to memorize silently and time or repetitions required to memorize orally. According to Shellow there is a higher correlation between recall and recognition for incidental memory than for direct memory. People who recall incidental material well are also likely to recognize it well.

Phenomenal Memorizing. Byrd (**8**) has reported the case of a feeble-minded negro. The chronological age is twenty-four, the mental age between eight and nine. The memory span is five digits. This negro gives days for dates and dates for days between 1901 and 1924. He also names many locomotive numbers, counties and county seats, states and capitals. His skill seems to have been acquired largely through the memorization of the contents of a notebook which he keeps. Hegge (**22**) discusses two phenomenal memorizers and their methods. Neither uses a cut-and-dried memory system. One does use, in memorization, a special knowledge of number combinations, but the other has a flexible method that might be copied by ordinary persons. R. Henning (**26**) discusses his own remarkable memory for dates, the uniqueness of which was not discovered until his thirtieth year. In his forty-ninth year, the time of writing, he finds that his memorial capacity has not diminished.

Memorizing Ability of the Blind and the Seeing. Vértes (**58**) studied the immediately memory of blind and seeing children. In the blind group there were 20 children all of whom had been blind from birth or early infancy. This group is compared with a group of 100 seeing children. It is clear from his results that the immediate memory of the blind children for material given an auditory presentation is better than that of the seeing. If Vértes had been content with that generalization, he would have remained on relatively solid ground, but evidently he could not be satisfied with such a simple outcome. He divided his 20 blind children into groups according to age, sex, and social status. The greatest number (5) of any one age were eight years. Eleven were boys, 9 were girls. Fourteen were poor, 6 were well-to-do. Without a symptom of statistical conscience, he has drawn the most reckless conclusions

from data resting upon such a flimsy basis. Twenty-five years ago this sort of thing might have been excusable.

II. Retention

The Curve of Retention. Luh (33) measured retention of lists of 12 nonsense syllables after 20 minutes, 1 hour, 4 hours, 1 day, and 2 days by the methods of anticipation, relearning, written reproduction, reconstruction, and recognition. An effort was made to eliminate the chance factor in the reconstruction and recognition scores. Luh furnishes tables of what he considers proper reconstruction and recognition scores for various combinations of right and wrong answers. The recognition formula earlier used by Strong is subjected to severe criticism by Luh who claims that that formula makes correct allowance for chance only in the event of extremely high and low scores.

All of Luh's retention curves for just learned material display the negatively accelerated form obtained by Ebbinghaus and most later investigators. While two inversions of the second order are present, there are no inversions of the first order such as Ballard secured for children. That is, while there were two irregular changes in rate of forgetting in the Luh experiment, there were no instances in which more was recalled later than earlier. The different criteria of retention employed yield different curves. Four of these stand invariably in a given order. Recognition gives the highest values. Reconstruction occupies the second position. Written reproduction follows as a close third. Anticipation always has the lowest value. The values for relearning cut across those for written reproduction and reconstruction.

Jones (27) found the usual rapid initial decrement for the retention of the contents of 40-minute lectures. From 62 per cent in immediate recall there was a sharp drop to 45 per cent after 3 to 4 days, followed by a slower drop to 24 per cent after 8 weeks.

Some years ago Ballard obtained retention curves which, as we stated above, showed better retention some time after memorizing than immediately after. Although, as Luh points out in reviewing Ballard's work, his data give every evidence of being valid, his findings are not in line with those of other experimenters. A recent study by Nicolai (39), however, points in the same direction. This investigator took his method from the work of Gotschlich and Griesbach. A number of common objects were exposed for a brief period and the subject recalled as many as possible immediately, later, or on

a number of occasions. In the first experiments where later recall proved more efficient than immediate recall the objection might be raised that, since the later recall came after the more immediate, it therefore had the advantage of what amounted to extra study. This objection cannot be sustained though, because Nicolai introduced other check experiments where this possible effect of recall was under control. In these experiments, too, he found higher retention values in remote than in immediate recall.

Effect of Degree of Learning upon Retention. In a second experiment Luh had his subjects obtain various degrees of mastery over lists of 12 syllables. These degrees of learning are represented by the percentages 150, 100, 67, and 33. One hundred and fifty per cent learning was secured by giving the subject half again as many repetitions as were required for 100 per cent mastery. The other degrees were secured in the same manner. Written reproduction, recognition, and reconstruction were the methods of measurement. The amount of retention for most intervals varies with the degree of learning, but the differences between the curves for 33 per cent and 67 per cent are greater than those between curves for 67 per cent and 100 per cent, and the differences between the 100 per cent and 150 per cent curves are least. In general the curves for the different degrees of learning are more like that of Ebbinghaus than they are like those of Ballard and Nicolai.

Tolman (57) reports that the more the elements in a list are overlearned the more easily the list will be relearned 5 minutes later. The effects of overlearning are not similarly advantageous after one week. Tolman suggests that overlearning may affect the strength of the perseveration tendency established rather than the strength of association, and that perseveration, while important for early recall, may be of practically no consequence later. This paper contains the description of a piece of apparatus which makes it convenient for the subject to learn lists of material by the *anticipation* or *prompting* method without having an experimenter there for purposes of recording the performance on each repetition.

Repetitions and Retention. Mibai (34) repeated in slightly modified form the Ebbinghaus experiment upon repetitions and retention. Ebbinghaus, it may be remembered, used 16-syllable lists which were given 8, 16, 24, 32, 42, 53, or 64 readings and relearned 24 hours later. The score was in terms of saving. Mibai used 12-syllable lists which were given 2, 4, 6, 8, and 10 repetitions and then relearned after one hour. Six subjects served in this experi-

ment. In the Ebbinghaus experiment it was found that there is a linear relationship between repetitions and saving. Each three repetitions in learning caused a saving of one repetition in relearning. Mibai verified this law. Although the individual results are irregular, the composite indicates a linear function. Each repetition in learning saves approximately 0.4 repetitions in relearning one hour later.

Distribution of Effort and Retention. Austin, in the study already mentioned, found that the advantage of relatively distributed effort in memorizing logical material is very much greater two weeks or a month after study than it is the next day. In other words, the distribution of effort affects retention as well as the process of memorizing. Robinson, in his study of distribution, secured results similar to those of Austin. Where the study of a list of 10 three-place numbers was concentrated into one sitting of 6 readings or divided into two sittings of 3 readings each, the concentrated study proved more efficient as tested by recall 5 minutes after the last reading. But by 20 minutes afterward this superiority of the concentrated form of study had been lost and by 24 hours afterward the distributed study was clearly better.

Effect of Fatigue During Memorizing upon Retention. The subjects in Morgan's experiment who spent about 4 hours studying the English equivalents of 850 German words were brought back after two days for recognition, recall, and relearning tests. The recognition scores for material in the five consecutive sections of the learning period were 5.47, 4.34, 3.66, 3.16, 2.79. The recall scores for material learned in the same periods were 0.40, 0.23, 0.17, 0.22, 0.21. The relearning scores as percentages of the learning scores were 89, 81, 78, 74, 71. Thus it appears that material learned late in a prolonged period of learning is not nearly so well retained as that learned earlier in the same period. Jones found no difference in the amount of retention for material from the first and second half of a lecture hour.

Effect of Recall upon Retention. Jones found that an attempted recall immediately following study has a pronounced influence upon the decrement in the curve of forgetting. He compared the effects of such recall with those of an extra impression of the material memorized. Retention tests on lecture material and associated words revealed a superiority of from 35 per cent to 50 per cent where there had been a recall, immediately after study. Brown (6) has made an interesting analysis of the effect of recall upon retention. One hun-

dred and ninety-four students were asked to write in five minutes the names of as many states as possible. One-half hour later, without warning, they were asked to do the same thing again. The results can best be expressed in a simple table:

Average number states remembered first test	36.31
Of these there appeared in second test	34.37
And new items appeared in second test	5.29
Making a total for second test	39.66

A similar experiment on the recall of a list of 48 unconnected common words yielded comparable results. Brown suggests that repetition tends to fix whatever appears in the first recall and that the chance setting of the second recall introduces more new items than are forgotten since the first recall. Those items of the first recall which come up only because of what might be called temporary or chance factors are likely to occur again in the second recall, even though the chance factors which originally accounted for them are absent, because the repetition which they get in the first recall tends to fix them.

Affective Tone and Retention. The relationship between the emotional character of experiences and their retention is a compelling subject of investigation. This is due in no small degree to the importance which psychoanalysts attach to the conception of *repression*. According to Smith (**54**), who detected the presence or absence of affective tone by means of associative reaction times and galvanometric deflections, words that are better remembered and words that are less well remembered have a higher affective tone than those which are moderately well remembered. He does not commit himself in regard to the precise psychological nature of the affective tone or tones dealt with. Rudlowski's (**48**) experiment on testimony has some bearing upon the effects of intense excitement without specification as to its position in some dimension of affectivity. The subjects of this investigator made many more errors in their testimony than those of Stern and Borst and Brenkink had made. Rudlowski explains this as due to the fact that in his work the scene upon which testimony was taken was really acted out. The others had used pictures.

Griffitts (**21**) attacked the more conventional problem of the relative retention of pleasant, unpleasant, and indifferent materials. That the best retention is of pleasant material seems clear, but the positions of unpleasant and indifferent material in respect to each other are

not so well established. The odds are somewhat in favor of the unpleasant words. Griffitts introduced a rather novel element into his experiment in that the words whose memory values were measured did not have their affective tone estimated directly. These words were used in association tests and the affective tones of their associates were estimated.

Berliner (5) studied the relation between affective tone and recognition. Seven groups containing about 20 subjects each ranked according to beauty pictures of buildings, pictures of people, and pictures illustrative of children's songs. Seven coefficients of correlation were calculated between esthetic ranks and errors of recognition. They ranged from —.73 to +.24. Six of the seven were negative. This indicates a better retention for the pictures judged more beautiful. The fact is particularly striking when it is realized that the subjects, in making up their rank orders, were directed to pick out the most beautiful first, then the next most beautiful, and so on. This means that the least beautiful were examined more frequently than the most beautiful. The least beautiful of all the pictures in a series, for example, had the possibility of being observed at the time every rank was decided upon. Gordon, some years before, had found no relationship between recognition memory and affective tone. Gordon's material, however, consisted of geometrical figures, and it seems probable that the differences in affective tone involved were very much less than those in the Berliner experiment.

Fox (18) had his subjects learn sonnets by the part method or by the whole method. Indications of pleasantness where given. There is a difference between the retention of the preferred and unpreferred sonnets. The preferred ones were better retained.

Wohlgemuth (65) takes account of the fact that a difference in the relative number of pleasant and unpleasant experiences may determine the number of each which one can recall at some later time. He asked school children, just after a holiday, to write down a list of pleasant experiences which occurred on the holiday and a similar list of unpleasant experiences. Ten days or two weeks later the same subjects were asked again to list the pleasant and unpleasant experiences of the day in question. After eliminating the experiences recalled for the first time in this second recall and figuring the ratio of remotely recalled experiences of a given affective tone to immediately recalled experiences of the same tone, the conclusion is reached that there is no difference between the retention values of experiences of opposite affective tones.

Washburn, Field, and Wolf (60) present facts in regard to the retention and recall of experiences of various specific emotional types. Ninety college girls were used as subjects. The directions asked for the recall of an occasion on which the subject was angry, afraid, or joyful. The reaction times were recorded and the subject rated the vividness of her recalled emotion on a scale of four steps, ranging from as vivid as the original emotion to mere remembrance that the emotion had been experienced. In comparing the results from different emotions, account was taken of the recency of the experiences recalled in order that a tendency toward recency or remoteness of one of the emotions would not affect judgments of its retainability. Joy was found to be more intensely revived than anger and fear. Values for the experiment as a whole are: joy, 168; fear, 108; anger, 104. Joy is the most quickly recalled and anger the most slowly recalled according to this investigation. It is pointed out that these results may to a large degree be a function of the youth, health, and generally fortunate status of the subjects. Washburn, Deyo, and Marks (59) repeated the experiment described above. Two new emotions, pride and shame, were added to the previous three. The first set of findings was in general confirmed. The average intensity of recall and average quickness of recall are high for pride. It is close to joy in these respects. While the average vividness of shame is not high, there is no other emotion having so many recalls which are equal in intensity to the original experience.

A very ingenious experiment was performed by Laird (30). From self-judgments and judgments of their fellows a group of 62 subjects was divided into three parts, one of which contained 23 presumable optimists, another 7 presumable pessimists, and a third 32 of mixed optimistic and pessimistic temperament. These subjects were asked at the beginning of a class hour to write down the names of ten people whom they personally knew. At the end of the same hour each subject made up a scale of ten steps containing the person whom he liked most first and the person whom he liked least last. Other names were entered at the intermediate positions in the scale. The experimenter then examined the spontaneous recalls made at the beginning of the class hour with a view to determining the appearance in that spontaneous recall of names included in the rating scale. He discovered that for the group as a whole the most liked person according to the rating scale appeared several times as often in the spontaneous recall as did the name of the most disliked person. Further study, however, revealed the fact that possibly this was more

a function of the predominant temperament of the group than of the mere pleasantness or unpleasantness of the names recalled. While the presumable optimists recalled the names of the most liked people very much more often than the names of the least liked, the mixed group did not show this tendency so strongly, and the presumable pessimists showed the opposite tendency. Laird's experiment undoubtedly needs repeating, but in the mind of the reviewer it marks a definite step forward in the investigation of affective tone and retention. Accumulated evidence makes clear that with groups of subjects optimistic recalls are more frequent than pessimistic recalls. Laird has sought to discover not only *whether* this is so, but also *why* it is so.

Length of Material and Retention. The experiments of Robinson and Heron and of Robinson and Darrow confirm the run of previous results in indicating a relatively slower forgetting for longer materials. In the nonsense syllable experiment the retention period was 20 minutes and in the number experiment the retention period was 15 minutes. In both cases there were inversions of this relationship within individual cycles and also in the tables giving averages by different criteria of retention for the experiments as wholes. Nevertheless the general trend is evident. It may be made out, too, that within the limits of 6 to 18 syllables and of 4 to 10 three-place numbers there is a negative flexion in the curves representing the length-retention relationship.

Jost's Law as Measured by Recognition. Keller (**28**) found that, although nonsense syllables are as well or better recognized than words after ten minutes, words are better recognized after 24 hours. He believes this is due to the fact that the words, being known before the experiment, are to a certain degree older associations. His results, looked at in this light, are a demonstration of Jost's law, according to which older associations are less rapidly forgotten than younger ones. In other experiments certain nonsense syllables were learned before the experiment proper began. These, when relearned, were better retained than syllables which had been learned for the first time. There is another phase of Jost's law to the effect that older associations profit more from an equal number of repetitions than do younger ones. The results of still another experiment by Keller seems also to confirm this point in the realm of recognition memory.

Retroactive Inhibition. As a result of their investigations of retroactive inhibition Müller and Pilzecker made two particularly

important statements in regard to this phenomenon. They claimed that the degree of retroactive inhibition is independent of the similarity of interpolated work to material the memory for which is affected. They claimed, further, that work introduced immediately (17.2 secs.) after memorizing has a more marked retroactive effect than work introduced at a later time (after 6 minutes). Upon these facts they based their explanation of retroactive inhibition. They held it to be due to the interruption of a perseveration or setting-in process rather than to association in the ordinary sense. DeCamp accepted the work of Müller and Pilzecker and sought to refine our knowledge of the phenomenon in question. His results are disappointing, due mainly to the fact that the evidence for the mere presence of retroaction was in his experiment quite scanty. He himself felt that this was due to the relative dissimilarity between interpolated work and original memorizing. Heine and Tolman got positive results with dissimilar interpolation and Webb in his monograph on animal learning presents positive results for experiments where the retroactive effects of maze learning on maze learning were studied. Brockbank, on the other hand, obtained negative results with dissimilar interpolation applied to animal learning.

This was the state of affairs in regard to the relation of similarity of interpolated work to original learning and the degree of retroactive inhibition at the beginning of the period under review. Robinson (**44**) took as his primary problem the relationship just mentioned. He conducted three experiments on this point. In the first two the originally memorized material consisted of lists of numbers. The interpolated activities consisted of memorizing similar lists, memorizing lists similar in content to the original material but differing in arrangement, memorizing consonants, memorizing poetry, written and mental multiplication, observing pictures, and reading simple prose with no attempt to memorize. Contrary to the claims of Müller and Pilzecker, it was found that studying a second set of material very similar to the original set has far greater retroactive effects than any less similar interpolation. There is some slight reason to believe that the reading of simple prose had less retroactive effects than the other forms of interpolation. There is no real basis for discriminating among those other forms of interpolation in regard to their retroactive influence. The third experiment was one suggested by the work of DeCamp. The subjects studied an arrangement of chess men. The period between this study and recall contained the study of a new arrangement of chess men, mental multipli-

cation, or reading prose. The interpolated conditions stand in the order named in so far as their retroaction is concerned. These experiments clearly establish a relationship between similarity of interpolation to original memorizing and the degree of retroactive inhibition.

In the monograph discussed in the paragraph above an experiment is described which deals with the second principal claim of Müller and Pilzecker, *i.e.*, the claim that by 6 minutes after study the effectiveness of a retroactive agency becomes much reduced. The plausibility of their theory that retroactive inhibition is due to the interruption of a physiological setting-in process of short duration rests upon the validity of this generalization. Müller and Pilzecker had compared the retroactive effect of interpolated work introduced after 17.2 seconds with that of work introduced after 6 minutes. They had used one subject in this comparison. Robinson compared the effects of interpolated work introduced after 20 seconds, 5 minutes 20 seconds, 10 minutes 20 seconds, and 15 minutes 20 seconds. Five subjects were used. There was considerable retroactive inhibition under each condition, but no significant differences between the conditions. Further work needs to be done on this point. It seems likely to the reviewer that, in the long run, the placing of interpolated work either just after learning or just before recall would show more retroactive inhibition than placing the interpolated work in some more intermediate position. Aside from the slight evidence offered by Müller and Pilzecker, there is as much reason to believe that the interference is a progressive one carrying over into the process of recall as there is to believe that a setting-in process is interrupted.

Heine had tested the relative susceptibility to retroactive inhibition of materials learned with varying degrees of thoroughness. There were certain complicating factors in her work, but in general it appears that the greater the degree of learning the less the susceptibility to retroactive inhibition. Robinson presented lists of numbers 4, 6, 8, 10, or 12 times and subjected such lists to a constant retroactive influence. The results are irregular. The inhibition according to amount of recall decreases with increasing learning and the inhibition according to errors increases with increasing learning. Results according to speed of recall do not show anything like a continuous variation. First there is a decrease in inhibition with increasing learning and then an increase.

In the Robinson-Heron and Robinson-Darrow experiments tests were made of the influence of length of material upon its suscepti-

bility to retroactive inhibition. With both the syllable material and the number material the evident trend is toward decreasing susceptibility to this form of inhibition with increasing length. But in both experiments there appears evidence for a lower limit to this law.

Prolonged Retention. Worcester (67) learned 12 prose selections from auditory presentation and 9 from visual presentation. The selections were relearned about 5 years later. The saving was approximately equal for the two methods of presentation. For one it was 44.44 per cent and for the other 43.09 per cent. The high degree of saving is very likely due to the fact that retention tests were made 1, 2, and 7 days after the original learning. Such experiments as those of Jones and of Brown, which we have already discussed, indicate what a pronounced effect attempted recall has upon retention. Titchener (56) has published a paper which describes retention tests upon material learned 46 years before. On his tenth birthday the author recited without error over 200 lines from Milton's *Hymn on the Morning of Christ's Nativity.* The poem was not reread again for 46 years. The rereading which then took place was from the same book used in the first memorizing. During the first rereading 11 lines were definitely familiar and several stanzas were vaguely familiar. Several stanzas were definitely strange and the rest of the poem was lacking in either familiarity or unfamiliarity. Practically complete relearning required only 14 readings, some of which contained recitation and prompting. This remarkable relearning performance is to be explained, in part at least, to the fact that the subject has accustomed himself to rote memorizing.

III. Recall

Question Answering Compared with Narration. Cady (9), working under Gault, made a study of testimony as given in answer to questions and as given in a narrative of the occurrences reported upon. ". . . more errors occur when the Ss are forced to answer questions than when they are free to choose their own details. Facts mentioned in narrative reports are apt to be mentioned correctly either in general or in detail, while reports made in answer to a list of questions will be affected by the range of the list and by the directness of the questions." Cady recommends as the most satisfactory method of obtaining accurate testimony a combination of the narrative and question-answer methods, with the narrative preceding.

Effect of Oath on Testimony. In an experiment by Rudlowski an exciting incident involving the apparent injury of a man by a live

wire was acted out before a class. Eleven questions were put to the class immediately and some time after the incident. The answers were with and without oath. The results indicate that testimony with oath is fuller and more accurate in both immediate and remote recall.

Constancy of Conditions in Impression and Recall. Floors took his method from the earlier work of Lambrecht who studied the effects of color and form on memory. In Floors' experiment the subjects memorized geometrical figures of various sizes. The following three conditions bring out the nature of his problem:

(A) Figures were of constant size in both impression and recall tests.

(B) Figures varied in size during impression and in test appeared in still another size.

(C) Figures were constant in size during impression, but in test they appeared in another size.

The outcome of these experiments shows the best recall where size is constant for both impression and test, and that it is better to have variation in both impression and test than in impression alone.

Laird, Remmers, and Peterson (31) used three kinds of paired material: Anglo-Saxon words and English equivalents, pairs of 3-place numbers and sums, obscure historical events and dates. In Organized Presentation material of the same kind was grouped together. In Unorganized Presentation materials of different kinds were mixed. Recall tests, by a form of the method of paired associates, were also Organized and Unorganized in the above sense. The best performance occurred where both impression and recall were organized. Where recall was unorganized it did not appear to make so much difference whether impression was organized or not.

IV. Qualitative Studies

O'Brien (40) claims that, so far as the determination of the effect of mode of presentation on learning is concerned, "mere objective data will do little more than indicate the most efficient mode of presentation for a particular O, until account is taken of the ideational type of the O's the attentive selection that they exercise among the various presentative aspects of a material, and the manner in which one sensory mode is subject to translation into another." In her own study she concentrated her efforts upon the introspective method. None of her subjects was able to learn a list of syllables or words if he succeeded under instruction in inhibiting vocimotor

imagery from the start. She found that the mode of presentation is not at all indicative of the modality of imagery that will be employed in either learning or recall. When recall was difficult the subjects reported the presence of a schema in one of three types: (a) visual, (b) kinesthetic, (c) rhythmic.

The two subjects of Wheeler and Cutsforth (62) were both blind. One was synesthetic; the other was not. The problem was to determine the rôle of synesthesia in learning. The second subject, therefore, served as a check. Two sets of nonsense syllables were employed, one for tactual presentation (Braille letters) and one for auditory presentation. Synesthetic phenomena appeared during the learning, retention, and recall in the case of the synesthetic subject. Visual synesthesia delayed the process of learning. One among a number of reasons given for this fact is that a syllable might be represented only by the color of the dominant letter.

H. Henning (25) believes that smells form associations with feelings, moods, and the like, more readily than with intellectual processes.

G. K. Adams (2) reports observations upon memory colors or, as she prefers to call them, imaginal overlays. Hering and Katz have pointed out how we tend to see objects in those colors in which we are used to seeing them. Stumpf and Henning have noticed similar facts for tones and odors. Adams conducted a systematic investigation of phenomena of this type. She has brought together in her paper a particularly fine set of observations from everyday life. She also gives accounts of experiments by means of which she was able to produce the overlay effect for a number of modalities.

Dimmick (11) made an introspective study of the memory after-image in audition. Titchener had previously suggested that the memory after-image is often met with in the taking of dictation, and Dimmick utilized dictation as activity for his O's. Dimmick concludes that "in any classification of images the memory after-image belongs where its name implies, between the after-image and the memory image. Whether it should be placed nearer the after-image or nearer the memory-image depends upon differences between individual O's."

Gösser (19) has again entered upon the discussion of the criteria for distinguishing the various levels of imagery, from the after-image to the ideational image. Gottheil (20) and Busse (7) have made memory studies, the main interests of which center around questions of imagery.

Crosland (10) made an introspective attack upon the problem of forgetting. He says that "the phenomena of forgetting result from the operation of at least two somewhat reciprocal processes, namely: a process of disintegrating and losing of details with a characteristic typifying of contents—this process we call dissociation . . .; and a process of subjective selecting, interpolating, and clarifying with its characteristic mechanisms of condensation, transposition, and elaboration—this process we call assimilation." And then, ". . . forgetting itself, appearing in the forms of dissociation and assimilation, is responsible for both the character of dream images and the character of memory images and images of imagination." The significance of this discovery in ordinary forgetting mechanisms of causes of what might be called the Freudian effects is quite clear.

In many ways Crosland's study is an admirable piece of work. But he who makes a qualitative study with avowedly qualitative methods may well watch his step in regard to quantitative generalizations. "The interpolative, constructive or additive process," says Crosland (p. 135), "progresses much more slowly than the destructive or subtractive process, and is largely dependent on it for existence. However, as the time elapsing since learning increases, and the effects of learning grow larger and more striking, the assimilative process moves with greater acceleration and with greater momentum." The reviewer has searched for the data which should support such quantitative statements, but the search has been rewarded by nothing more quantitative than the verbal reports of introspections. Is it possible that this talk about the acceleration of the assimilative process and the like is metaphor? Mere introspection, no more than mere statistics, endows one with a poet's license.

V. Memory in the Insane and Defective

Insanity is frequently accompanied by a quantitative impairment of memory capacity, but experimenters have not been highly successful in demonstrating alterations of a qualitative variety. Achilles found essentially the same differences between recall and recognition memory in the insane as in normal subjects. Liljencrants (32) concludes that the insane are affected much as are normal subjects by length of exposure and by such factors as primacy, recency, and repetitions. More positive findings are reported by Foster (17). While the insane, according to this investigator, give responses in the memory span for digits which are not markedly

different from those given by normal persons of the same mental and chronological ages, inadequate responses are frequent in memory for drawings and short paragraphs. In certain types of insanity there are significant characteristics of the memory responses which are not recorded in the quantitative scores. For instance, extreme sizes and irrelevant details occur in the memory drawings and numerous errors and additions in the reports of paragraph selections. Wells and Martin (61) present a number of memory tests for the insane. They express a hope that important distinctions between various groups of disorders may be detected through memory profiles.

Fildes (14), who has compared 50 defective boys with groups of normal children, makes the following statements about the learning of high grade defectives: They show (a) less power than normals to form other than very obvious associations, (b) a tendency to be confused rather than helped by chance associations that arise, (c) less power of criticism, (d) less ability to work under new conditions, and (e) a tendency toward automatism.

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RIGHT AND LEFT-HANDEDNESS

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A paper by Schwerz (**24**) sketches the many angles from which the problem of handedness must be canvassed. Proof is cited that prehistoric peoples used, for the most part, the right hand (a statement with which Thurnwald (**28**) is in agreement) but among them there were found a considerable number of left-handed and ambidextrous persons. Estimates of left-handedness among present-day Europeans range from 1 to 8 per cent, with probably a greater prevalence among males than females. Handedness cannot be determined from bone-measurements with certainty, although a strong presumption is established when all the measurements are harmonious. Inconsistent measures are common; the measurements of the humerus appear more significant than are measurements of the ulna or radius. Handedness cannot be determined by a test of the relative strength of the two hands. Attention is called to the greater expressiveness of the right side of the face in right-handed individuals. Speech defects are cited as extremely common in cases of left-handedness and ambidexterity. Many theories exist as to the origin of right-handedness. If it be due to superiority of the left-hemisphere, the question shifts to the origin of this superiority. Schwerz believes that lateralization of function occurred as a chance variation which had survival value particularly when it issued in right-handedness. Left-handedness has survived through inheritance of it, the principles governing its transmission being very complex. Some evidence exists that the left hemisphere is more highly organized than the right but microscopic examinations have made no contribution to the problem. Many sources of error are present in attempting to determine the relative weight of the two hemispheres.

Ganter (**6**) calls attention to the fact that many different sorts of dexterity exist. Prolonged observations of the inmates of institutions convinced him that many left-handed individuals escape casual observation. Using as a criterion the dominance of the left hand in bimanual operations, Ganter reports that 21.9 per cent of epileptics are left-handed and 18.7 of the feebleminded. Left-handedness is

so prevalent among epileptics possibly because of a slight encephalitis in early childhood, the right hemisphere taking over the functions of the left. Ganter reports an interesting study of right and left patellar reflexes in connection with handedness but with no positive results. He recognizes that left-handedness is transmitted in families, more commonly *through* the father and *to* the son but can see no evidence of regularity in transmission. His conclusion is that, in part, left-handedness is a sign of degeneration since it is so often associated with epilepsy, feeblemindedness, speech defects and facial paralyses; but that it may also be transmitted by a left-handed remnant of humanity. Skill in fine movements and greater strength need not be associated in the same arm nor do bone-measurements give unambiguous information as to handedness. The evidence at hand concerning the comparative weight of the two hemispheres is inconclusive. Ganter's own experiments indicate that in the majority of cases the *right* hemisphere is the heavier. Many sources of error in weighing the hemispheres are recognized. Ganter believes that the predominance of the left hemisphere is purely functional in nature and originated through natural selection of a favorable variation.

As reports on handedness multiply it becomes more and more evident that a more thorough-going classification of types of dexterity is needed and that apart from a statement of how hand-dominance was determined statistical reports have little significance.

Rife (22) recognizes six forms of dextrality, namely, three distinct types of right-handedness and three corresponding forms of left-handedness. This classification is based upon the way in which individuals carry out bimanual operations. Type A comprises those whose bimanual preference is divided between right and left; type B includes those whose bimanual preferences is the opposite of their unimanual preference, and type C embraces the individuals whose unimanual and bimanual preference is the same.

Kerr (13) defines three classes of functional attainment in dextrality. His division is based upon a somewhat theoretical position. Dextrality is described as "the specialized aptitude associated with the psychical phenomena of speech and reasoning which is so marked in the left brain." The three classes are the following: (1) The highest development of both hands in true ambidexterity. In this first class we find high intellectual and expressive attainments. (2) The ordinary development—one hand well developed in function, the other mainly accessory. The immense majority belong to this second class and with them right or left-handedness is an accident and not

expected to affect mentality. (3) Defective functioning of both hands, really an ambisinistrality which probably includes many so-called left-handed. This class includes many individuals who have not developed properly mentally and who show many degenerative stigmata such as speech defects.

In this connection a note from Hall's *Life and Confessions* (9) is of interest. Hall writes: "If my brain should be examined at my death I am sure that the left will be found to be less developed or more atrophied than the right hemisphere for the attacks of migraine to which I have been occasionally subjected are more severe on that side. On the other hand, the speech centers are there and my command of language is not deficient. I have, too, an unusual ambidexterity, being, however, fundamentally left-handed. Till I was eleven I wrote with my left hand and was then made to change, to the permanent detriment of my penmanship. I have taken drawing lessons but proficiency here has always been out of the question so that even the simpler diagrams for my lectures have been made on charts prepared beforehand under my direction. I bat, throw, and am strongest in the left arm and the right leg. Vision is strongest in the left eye and audition in the right ear."

Quinan (20) recognizes crossed forms of left-handedness. The familiar form may reveal itself in an individual through some deviation from the right-handed conventions. Crossed forms are latent and unless they are sought for they escape observation. Left-eyedness, for example, is not uncommon in the right-handed and, conversely, right-eyedness is common in sinistrals. The etiology of these mixed types is obscure. Quinan's groups include the pure dextrals and three groups of sinistrals. Group 1, left-handed, left-eyed, pure sinistrals; Group 2, left-handed, right-eyed, crossed sinistrals; Group 3, right-handed, left-eyed, crossed sinistrals. Quinan reports that high arterial tension occurs more frequently in left-handed than in right-handed people, that hereditary predisposition is a definite factor in the etiology of high blood pressure, and that high arterial tension is suggestive of constitutional inferiority. Stammering was found to occur in his sinistrals with a frequency from three to seven times greater than its occurrence among dextrals, thus strengthening his belief that the "disorders of speech so often noted in left-handed people are due to congenital inferiority of the central nervous system."

Carrying his investigations further, Quinan (21) reports on the prevalence of sinistrality in three groups of 100 men each. An

extraordinary contrast was found between two groups composed of musicians and machinists, men whose vocations demand a high order of muscle coördination. Four per cent of the machinists proved to be left-handed and an additional 4 per cent had sinistral peculiarities; 8 per cent of left-handedness was found among musicians and 24 per cent of sinistral peculiarities. This result suggested two questions: Do left-handed musicians enjoy a physical advantage in comparison with right-handed players? Does instrumental technique modify ocular dominance? Both questions are answered in the negative; muscle coördination is more highly organized in right-handed players.

Quinan concludes, "an intimate relation appears to exist between left laterality and a type of mental organization prevalent in musicians." Left-handedness and sinistrality are usually indicative of the psychopathic constitution. A high proportion of left-handedness among musicians is suggestive of mental instability. Sinistrals "feel drawn to the musician's vocation in spite of the fact that it is a vocation for which in many instances they are not well adapted." Quinan believes that left-handed and sinistral musicians are much more nervous and "temperamental" than dextral musicians.

A report of interest in connection with Quinan's is that of Sikes,(25) on musical talent and left-handedness. Twenty-two years of teaching piano and organ have convinced the writer "that unusually talented pupils play easily and by preference with the left hand; moderately talented pupils with the left hand with an ease corresponding to the right. The untalented use the left hand with difficulty, preferring the right which is more quickly trained." Sikes observes the left hand as a test for talent although she says that this use of the left hand has no connection with left-handedness in ordinary matters.

Joteyko (11) believes that right-handedness originates in the law of least effort. It is a defensive mechanism to save labor on the part of the heart by leading a man to use in difficult work the right hand only or both hands at once, sparing the left hand which by its position is more intimately related to the heart. Experiments conducted with Mlle. Kipiani have proved that intense work with the left hand produces greater effect on the heart than the same work with the right hand. Joteyko believes that physically it is more dangerous to be a pronouncedly left-handed man than a right-handed one and that left-handed men should not take up occupations involving great physical strain unless they become ambidextrous. In advising mutilated

soldiers, a change of occupation from that followed before the loss of a right arm is suggested if such work involved heavy labor for which the left hand must now be used. In other cases, it has been found advantageous to use the left hand in the same kind of work as that in which the right hand was formerly used since the left hand and arm have profited by cross-education. Such cross-education is more effective in the production of symmetrical movements. Joteyko, who considers ambidexterity most profitable, urges that aphasic patients should be practiced in mirror-writing with the left hand, and that practice in writing with the left hand should be prescribed for soldiers whose right arm has been amputated.

Pfeifer (19) after experimental production of mirror-writing in an illiterate man, and clinical observations on the differences in learning to write with the left hand by right-side paralytics and by patients with an amputated right arm advanced the theory that the incapacity of the left hand is not so much a native incapacity as an incapacity resulting from inhibitions originating from the habitual use of the right hand.

Liepmann (14), reviewing this article, finds nothing new in the explanation of mirror-writing as due to symmetrical exercise of the left hand by the exercise of the right and nothing new in the notion that there may be an inhibitory effect of right-hand exercise on similar or asymmetrical left-hand movements, but rejects vigorously the conclusion that the inferiority of the left hand is, in general, due to inhibitions arising from exercise of the left hemisphere. The inferiority of the left hand is actually assumed on the basis of just the symmetrical movements themselves.

Pfeifer (18) replies briefly that Liepmann's personal bias has led him to avoid the real issue raised in the former's paper. In a later article he handles the problems of handedness at considerable length but does not return to a closer consideration of Liepmann's criticism of his use of the clinical material cited by him in support of his general position. More evidence is given to show that practice with the right (or left) hand, however inaugurated, has an inhibitory effect upon identical movements by the other hand. The left hand is by no means as lacking in skill as the conventional view would have it; and the right brain is capable of assuming certain functions in case of their loss by the left brain. Fluent left-hand mirror-writing gives no evidence of any inferiority on the part of the right hemisphere although normal script when written by the left hand bears witness

to inhibitions due to the latent exercise of the left hand on symmetrical forms. On the other hand, a great deal of effort is demanded for production of elegant writing by the right hand. Great skill is shown by the left hand in piano-playing, although less in identical than in symmetrical movements.

Pfeifer cites cases to show that left-handedness in childhood is sometimes due to a pathological condition of the left hemisphere and that later this hemisphere may resume control. Leonardo da Vinci's case is discussed in great detail. Pfeifer believes that da Vinci was born right-handed; for some reason (peripheral or central) he was forced during his second decade to use his left hand. He discovered and used mirror-writing for a year. Later, after he recovered from this pathological left-handedness, mirror-script became a secret script for him and was then produced by the *right* hand. The somewhat similar case of Menzel, the painter, is also discussed. Pfeifer believes that in cases of pathological left-handedness, cortical lesions may heal and a shift in control from the right to the left hemisphere occur. This may easily take place during the first decade of life; later, lateralization of function becomes more thoroughly stabilized.

Schmolke (**23**) contributes a conservative estimate of the value of cultivated ambidexterity.

Myerson and Quercy (**16**) confirm the observations of other authors that spatial displacements of every sort occur in the first attempts of many intellectually normal and right-handed children at copying or drawing from memory digits, letters, familiar objects and the like. Spatial relations are not well constituted in the child's early perceptions. Visual and motor spatial facts are associated only through experience. Disorientation yields to appropriate motor education but frequent relapses appear during the child's development. An attempt is made to bring this doctrine of original perceptual fluidity into line with doctrines of spatial relativity (fourth dimension, etc.).

Fildes' experiments on mirror-writing (**5**) offers further evidence in confirmation of the conclusion that the causes of wrong orientation are many and by no means confined to the use of the left hand. A fairly large percentage of defective children are left-handed; they are more liable to make the mistake of right-left reversal than normal children, and because of the absence of criticism on their part, they repeat such mistakes if once made.

Fildes and Myers (**4**) in studying letter-reversal in a left-handed boy reach the same conclusions, observing that while the visual and motor schemata are being intergrated in learning to recognize and make a letter, confusion must arise from the tendency for one hand to perform the reversed (mirrored) movements learned by the other, a confusion accentuated in the case of a left-handed child whose right and left hands are both being employed in writing.

Dallenbach (**3**), reporting an experiment on spatial position as a factor in the determination of attention, concludes that position is a condition of attributive clearness and that the left-hand position and the position above have an attentional advantage, the position to the left and below the point of fixation being the most advantageous position of all. The reagents in his experiment were both right-handed. To confirm this experimental finding Burke and Dallenbach (**2**) repeated the experiment on two left-handed observers, one of whom proved to belong not to a pure left-handed type but to an intermediate type. They reaffirm that position is a condition of clearness and a determinant of attention and add that "specific preferential position seems to be a matter of right- or left-handedness; the positions to the left and above have a distinct attentional advantage for right-handed O's and contrariwise the positions to the right and below have an attentional advantage for left-handed O's."

Beeley (**1**) is interested mainly in the educational problems arising out of left-handedness; in tests for diagnosing handedness, particularly in young children; and in the relation of left-handedness to mirror-writing and the handling of the latter as a school problem.

Smith (**26**) summarizes seven theories of the origin of right-handedness. She found from a questionnaire investigation that of 2,055 school children, 4½ per cent of the girls and 5½ per cent of the boys were reported left-handed; "out of 500 delinquent children, 6 per cent of the girls and 11 per cent of the boys; out of 200 feeble-minded, 11 per cent of the girls and 8½ per cent of the boys; out of 500 deaf, 3 per cent of the girls and 4 per cent of the boys; and out of 200 blind, 11 per cent of the girls and 10 per cent of the boys."

Hollingworth and Taylor (**10**) tested the strength of grip for three groups of children (forty-five each) using Smedley's dynamometer. Seven of the group of gifted children had an equal grip in both hands; two had a stronger grip in the left hand. Four of the group of average children had an equal grip in both hands and six a stronger left-hand grip; and these results were duplicated in the group of stupid children.

Gordon (7) reports that the incidence of left-handedness in 1,019 dependent children to whom she gave intelligence tests, was 2 per cent for 658 girls, and 5 per cent for 361 boys. The left-handed children in this group proved to be not at all inferior in mental level to the right-handed. Some evidence was found for the theory that left-handedness is inherited. Miss Gordon's criterion for left-handedness was very simple; whenever the child took pen or pencil in the left hand, in carrying out the tests that require drawing or other graphic reaction, he was asked whether he always used that hand.

Nice (17) reports an interesting study of seven cases of delayed speech with special reference to the handedness of the children. These children showed a prolonged state of uncertainty as to which hand should lead. A possible explanation of "the coincidence of retardation in speech and in the ascendency of the right hand might be that as long as the dominant hand center was not definitely settled the speech center could not be located." Mrs. Nice recognizes, however, that coincidence of ambidexterity and delay in learning to talk does not occur in all cases. She urges the need of a large number of studies of children who are more or less slow in talking.

Thompson (27) and Gruenberg (8) call attention to right and left-handed speakers and the effect of "asymmetrical oratory" upon an audience or in the schoolroom.

Lutz (15) asks, How do you clasp your hands? Is the right or the left thumb uppermost? The manner in which the hands are clasped is, he claims, a matter of heredity and has apparently no connection with one's right-handedness or left-handedness.

Kempf (12), investigating the preferential use of the hands in monkeys and the modification by training, reports interesting individual differences both with respect to hand-dominance and the ease with which this dominance could be modified and the permanence of the modification.

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